CALIFORNIA INSTITUTE OF TECHNOLOGY
PASADENA
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KERCKHOFF LABORATORIES
OF BIOLOGY

March 3, 1951

Dear Josh,

This is just a note for several reasons - information from the Factory, some information desired from you, etc.

The following of interest is going on here. Rowley arrived from England via NY (Heidelberger's lab). He's a medical bacteriologist and is attempting to screen penicillin sensitive coli using K 12 for genetic reasons. He seems to have some of the med bacteriologists views (re Bellamy and Klimek, etc) but we'll wean him away. He is interested in the mechanism of penicillin resistance. Delbruck's group is the only other lab working on K 12 (Dubnoff has been working on Davis' methionine B 12 mutants biochemically and concludes that Davis' scheme is wrong). Weigle and Peggy are doing some lwoffing stddies on K 12 and lambda -just what I don't know. Vogt is working on strep resistant K 12 and something to do with picking up the zygotes. D. has been critical of my kinetics stuff on the grounds that (a) an induction period occurs in some time crosses and (b) Vogt couldn't repeat the stuff. Well, (a) agglutination does occur (I looked at the crosses under phase contrast) and the rate of zygote formation may be proportional to the degree of interparental agglutination resulting in some form of autocatalytic function that reduces to zero order kinetics. As for (b) V. was not using my methods (she allows recomb in nutrient broth followed by 1/100 dilution and plating in minimal, circa 3.5 ml suspending layer on 15 ml bottom layer plus streptomycin or something like that) and I have been able to repeat my stuff. You are right - Y24 requires bi tin when clean agar, etc. is used. Haas is working on histidine synthesis in Neurospora.

I've been working on crossing as a function of growth phase and medium conditions, and on s+ K 12. Not much to say about crossing conditions - early maximal is best and symgamy seems to parallel filament formation and agglutination. You may remember the s+ coli I got from K12 which grows in or on succ, fum, and malic acids while K 12 does not. I picked it up once but never again even after mass platings. (K12 grows in succinate agar if agar is present but still is s-, probably by Thunberg-Wieland split to acetate and further oxidation). To prove that it was K 12 required crossing - which has been done by sticking in two markers - histidine and phenylalanine. It crosses with Y 24 (different phenylalanine loci), 679-680 and derivatives with segregation of Lac and V markers, and with K12 s- auxotrophs but all recombinants are s+ (funny linkage or cytoplasmic factor). Karlsson analysis shows malic dehydrogenase (or malic enzyme of Ochoa) to be absent as does growth on tetrazol by your method. I'm working on acetone powders now to clear up biochem.

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The rest of the time has been spent kidding around with the latest fad - paper chromatography. Nothing is settled for next year and they probably won't renew the Gosney Fellowship. So I'm looking around for something else in the way of fellowships or even work. If you know any leads I would appreciate hearing about them.

No word has been received from Ryan for some time. The last was mainly about the quality of the '49 vines (of the best).

Some people here want your strains (679-680, Y24, W1205, Y9, 58-161) so is it OK to let them have them?

Sincerely,

Tom Nelson